The Behavior Analyst 1989, 12, 59–67 No. 1 (Spring)

Book Review

Questioning Psychology's Mechanism: A Review of Costall and Still's *Cognitive Psychology in Question*

Edward K. Morris
University of Kansas

What are we to make of an edited book that questions cognitive psychology, none of whose authors is a behavior analyst? Of a book that offers critiques of and alternatives to cognitive psychology, none of them behavior analytic? Of a book that contains chapters criticizing behavior analysis in a misinformed fashion?

Oddly, much is to be made of Alan Costall and Arthur Still's (1987) *Cognitive Psychology in Question.* First, the authors and editors criticize cognitive psychology on largely different grounds than do behavior analysts: They criticize it for being mechanistic in world view, which contrasts with, but supplements, behavior analytic critiques of mentalism and dualism. Second, the criticisms focus on specific difficulties within various cognitive programs, thereby offering new avenues for challenging those programs. And third, the alternatives offered vary widely, but are loosely, though not obviously, contextualistic in world view, as is behavior analysis (Hayes, Hayes, & Reese, 1988; Morris, 1988).

But perhaps we should not make too much of this book because, although the authors and editors have prepared a series of often interesting and insightful essays, their contributions are essays nonetheless. The chapters are not deep and substantive reviews. The book, then, is more primer and potpourri than handbook. Nonetheless, we are offered a broad introduction to shared concerns about cognitive psychology in a relatively compact volume (260 pages) that warrants our consideration.

The Cognitive Psychology in Question

Before the book can be fairly evaluated, we need to know what the authors and editors take the cognitive psychology in question to be. It is this: Contemporary cognitive psychology adheres to a computational model of human activity, whose structure is likened to an intelligent machine (e.g., a computer) and whose functions are likened to computational processes (e.g., information processing) (see Haugland, 1981). More focally, cognitive psychology is interested in the internal states of the machine, that is, in the hardware and software designs through which environmental input is represented, rules are extracted, and both are processed. Although the computational model is often no more than a heuristic device for conceptualizing human activity, it not infrequently is said to represent the mind. For cognitive psychologists who are more comfortable with materialism than with metaphor or mind, the computational model is a model of brain structure and functioning.

Introduction

In their introduction, Still and Costall explain that the book's coherence lies not so much in a common content, but in a common intellectual history of unspoken assumptions. Although identified in this opening chapter, those assumptions remain too much unspoken throughout.

Costall, A., & Still, A. (1987). *Cognitive psychology in question.* New York: St. Martin's Press (ISBN 0-312-00378-1) ($29.95). Bryan D. Midgely, Susan M. Schneider, and James T. Todd commented on an earlier version of this manuscript, but are not responsible for any remaining maladroitness of mind. Reprint requests should be sent to the author at the Department of Human Development, University of Kansas, Lawrence, KS 66045.

The first assumption is "anti-dualism": What we speak of as mind is not distinct from behavior, but rather is the quality of behavior in context that sets the occasion for our calling it "purposive" or "intentional" (Skinner, 1945). Contemporary cognitive psychology overlooked the early anti-dualistic critiques by the pragmatists and functionalists, in part because the source of those critiques was ascribed to a mechanistic behaviorism that was easily overturned, and with it, supposedly, the criticisms—but the criticisms remain cogent within behaviorism and without (cf. Skinner, 1977).

Deeper than anti-dualism is the assumption of "mutualism," whose historical lineage reaches back to James in psychology, Dewey in education, and Mead in sociology—the early pragmatists and functionalists. In mutualism, neither organism nor environment (stimulus or response) can be defined apart from the function of one for the other (cf. Skinner, 1935).

Deeper still is the "questioning of abstraction," in this case, the abstracting of "rules" from human behavior in laboratory contexts, and treating the rules as different stuff than the behavior—which is dualistic; as existing independently from the activity—which is not mutualism; and, worst, as explanations of that activity—which is tautological (Skinner, 1977).

Here, and throughout the book, we should be clear that the editors and authors are not questioning cognition as a domain of human activity for study. What they question is cognitivism as "dogma"—the dogma that all psychological explanations can be cast in terms of internal representations and rules. That said, let me turn to the first of the book's two sections, in which the critiques of cognitive psychology are offered.

Against Cognitivism

Skills and actions. The first of two subsections contains three chapters questioning the cognitive account of skill, action, and knowledge as rule-dependent. As presented by Dreyfus and Dreyfus, the cognitive account of skill and knowledge acquisition goes as follows: Learners first encounter particular cases (e.g., with respect to driving a car and playing chess) and learn from those particulars; with further learning, however, they abstract out and internalize rules derived from the particulars, rules that then become the source of an expert's skills and knowledge.

The Dreyfuses argue that just the opposite occurs: The skills and knowledge of novice learners are largely dependent on rules and heuristics; once they become experts, the skills and knowledge come under the control of particular cases, not rules. This may explain why, when experts are asked what rules they follow, their answers are often no more than a regression to the rules they may have followed as novices, but that they now no longer use. There is much here to support and supplement the behavioral analysis of rule-governed and contingency-shaped behavior (Skinner, 1969, pp. 133–171; Vaughan, 1987).

Mixon's difficult chapter makes a similar point. Contemporary cognitive psychology studies discretionary, nonsocial tasks in order to tell us something about the rules involved in similar activity outside of the laboratory (e.g., dialing a telephone number), but the latter are largely uninteresting. Behavior is interesting when the *way* it occurs is important—the way we socialize, the way we speak, the way we think. We acquire these "ways" through (contingency-shaped) experience which, in turn, determines whether we can behave in a certain way, or will. Even if cognitive psychology can describe certain abstract rules of knowledge, or even if we can describe our own knowledge in those terms, that knowledge does not go very far towards explaining what we do and the ways in which we do it. For instance, although we may "know" the rules of proper dress and decorum for an embassy ball, knowledge formally construed does not mean we can behave properly, or will. Social behavior, indeed, our very "selves," are poorly accounted for by cognitive psychology.

The chapter by Shotter is an oddly placed and quirky essay which addresses several relationships among cognitive psychology, politics, and economics. Shotter's main point appears to be that abstracting out the rules underlying skilled worker behavior not only mechanizes those skills conceptually, but also overlooks important motivational issues and reduces the worker's status within the socioeconomic system. These points are related to F. W. Taylor's views on the scientific management of the workplace: his "movement" to mechanize industrial activity, and thereby render "work independent of craft, tradition, and the workers' skills" (p. 45), such that people can be replaced by machines. Shotter sees the artificial intelligence movement as part of a "people replacement" business that will manufacture unemployment. These are consequences of cognitive psychology that behavior analysts for social action may wish to address, and they raise some interesting issues with respect to the controlling variables over rule-governed versus contingency-shaped behavior, but the chapter has the unhappy tone and logic of Schwartz, Schuldenfrei, and Lacy's (1978) argument that operant psychology produced factory psychology.

Intentionality and classification. In the first of the two chapters in this second subsection against cognitivism, Palmer addresses "Hume's problem." Put in contemporary terms, Hume's problem is that the postulation of rules and representations by which we know the world presupposes that someone exists to use those rules and representations—rules and representations have no meaning without someone to act on them intentionally. Yet, who is this someone? If all our knowledge is grounded in these rules and representations, then so is our knowledge of the someone who uses the rules and representations. But we do manifestly "know ourselves," so another knower—a homunculus—must exist independently of the rules and representations, using them intentionally to inform us about our world and ourselves. If so, then how does that knower know? On the basis of the same representational and rule-governed scheme? If so, then how can that knower know itself? It, too, must have an independent, intentional knower. The regress is infinite.

Contemporary cognitive psychology is said to obviate these difficulties by analogizing minds and computers, the point being that computers *work,* and they work without having little "intentional" computers inside of them to use the rules and representations. In this view, what makes computers work are little "unintentional" computers or mechanical homunculi (e.g., switches) beyond which there is no regress.

Palmer sees the issues differently and correctly: Cognitive psychology has *not* solved Hume's problem, for the issue of intentionality remains. Computers simply do not display the intentionality of biological organisms. They are built from entities that are not themselves intentional, and at no point can intention emerge from unintentional objects, no matter how complexly they are constructed. Intentional behavior remains, as it always has, not within the purview of cognitive psychology, but within the purview of operant behavior: "Operant behavior is the very field of purpose and intention" (Skinner, 1974, p. 61).

In the next chapter, Ghiselin points out that much of science, psychology, and everyday life involves "classifying" and "classification," but his language and locutions make the arguments sometimes difficult to follow. As for "classification" in its nominal form or "classify" as concrete behavioral activity, these are not especially troublesome. A "classification" is something like "a formalized stimulus class," as in an organized class of stimuli that share some properties, relationships, or functions (e.g., animal-vegetable-mineral; periodic tables; botanical taxonomies; operants and respondents; reinforcers and punishers; different words having similar meanings). As concrete behavioral activity, "classify" has the sense of something like "act upon with respect to common properties" (e.g., labelling consequences that increase the frequency of responding as "reinforcers" and those that decrease re-

sponding as "punishers"; placing cumulative records showing scallops in one folder and those showing break-and-run patterns in another).

"Classify," however, presents problems when it appears in locutions suggestive not of behavior, but of a process, as in when someone says, "The researcher classified the consequence as a 'reinforcer,'" when all the researcher did was say, "That's a 'reinforcer.'" In this locution, "classify" implies something more than the mere tacting of a behavioral relationship, even though that may be all that it means.

Admittedly, "classify" appears in this locutional structure in a form identical to the familiar "to discriminate" or "to generalize," as in "the organism generalized (or discriminated)," with which we have fewer problems.[1] We generally know what we mean here, even if the locution is wrong for implying that "discriminating" and "generalizing" are organism-based processes, that is, that they emanate from or are governed or caused by the organism. Just like "discriminating" and "generalizing," "classifying" may mean nothing more than "behaving with respect to a stimulus class" (e.g., calling a response consequence a reinforcer as opposed to a punisher; referring to a cumulative record as scalloped). This sense of "classify," however, is redundant with already extant principles, and hence superfluous. Worse, though, not only is "classified" an "unprincipled" term, it is also unfamiliar in our lexicon, which further invites its misunderstanding as an organismic, perhaps even a cognitive, process (see Hineline, 1984; Morris, Higgins, & Bickel, 1982).

With that as prologue, Ghiselin makes the following two points. First, classifications are too often presumed to be absolute, timeless entities unto themselves, often immutable in character. Second, in cognitive psychology, classifications are studied in order to understand classifying as a form of behavior. Ghiselin's "evolutionary" perspective suggests that this is backwards: Studying classifications in order to understand classifying yields a circular explanation, thereby affirming the consequent of how things must be, to be so classified. If we are to understand classification, Ghiselin writes, we must study the behavior of classifying, and study it as a historical, evolutional process. In so doing, we will find that classifications are not based so much on physical similarities, but rather are a function of "what things do" and "where they come from"—in other words, useful classifications are based on functional, not structural properties (cf. Catania, 1973, 1978). Ghiselin is clearly behavior-analytic in his thinking here, as has been pointed out elsewhere (Skinner, 1988).

Alternatives to Cognitivism

In their prefatory material to the second part of their book, Costall and Still note that cognitive psychologists, too, have acknowledged some of the limitations described above. Those acknowledgments, though, lead only to repair and redesign, not to replacing the overall model, because the only apparent alternative is to return to a "mechanistic behaviorism." Not so, Costall and Still insist: Other alternatives exist, the best known of which is James J. Gibson's ecological approach to psychology, four chapters on which occupy the middle third of the book.

J. J. Gibson's ecological approach to psychology. The first and fourth chapters in this subsection were prepared by Reed, a noted Gibson scholar. I treat these chapters together, for they share some of the same difficulties. In his first chapter, Reed asks, "Why do things look as they do?" and answers it this way: "Gibson says things look as they do because they afford what they do, that we have evolved so as to perceive affordances and there is optical information available specifying those affordances" (p. 111). Gibson's

[1] This problem at the stimulus-control end of the three-term contingency is similar to a problem at the reinforcement end—the "reinforcing the organism" problem. Responding is reinforced, not organisms. In turn, organisms do not discriminate or generalize; rather, their responding is a function of stimulus control relationships. Both locutions are misleading.

psychology is not easily revealed in this passage, nor anywhere else in this section—or at least was opaque to me.

Negatively speaking, what Gibson's psychology is *not* was clearer: It is not representational and it is not theoretical in the sense of appealing "to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1950, p. 153). A more specific, but again negative, answer to Reed's question: The reason we see as we do is not because the mind interprets brain activity and not because the brain represents, computes, and gives meaning to incoming information. As Costall (1984) pointed out in a review of Gibson (1979), this is exactly what behavior analysis is not, too.

Before a positive answer to "Why do things look as they do?" can be offered, we need to know what Gibson means by "look." Does he mean "look" in the sense of "physical appearance," as in "Why do things physically *appear* as they do"? Probably not, for Gibson-the-psychologist was not overly concerned with the physical optics and sensory neurophysiology of vision. Does he mean "look" in the sense of "function," as in "Why do things *function* as they do"? Yes, that is closer; at least it concurs with Costall's (1984) comment about Gibson's view: "We can properly be said to immediately perceive the *functions* that objects serve for our actions" (pp. 112–113) (emphasis added). Quoting from Gibson (1979), Reed states: "The perceiving of an affordance is not a process of perceiving a value-free physical object to which meaning is somehow added . . . it is a process of perceiving a value-rich ecological object" (p. 140). That is, the functions of the objects for perceivers are what the objects "afford for behavior."

This wording unfortunately casts "perceiving" as a hypothetical process mediating environment and action, whereas "perceiving" is better taken to mean "behaving with respect to the function of." Gibson was clear that we perceive directly, not indirectly; that is, we do not perceive on the basis of intervening mental sensations and representations. Gibson's theory is thus a theory of direct perception, just as Skinner's is a theory of "direct behavior." For instance, stimulus functions (e.g., reinforcing and discriminative) and response functions (e.g., operants) define one another and are mutual—nothing mediates their interaction.

Reed's main criticisms of cognitive psychology are familiar, but offer an interesting twist. First, he objects to cognitivism for having only "improved" S-R psychology by adding mental representations and a hypothetical physiology to it, both of which "turn stimulus inputs into knowledge, and organise responses meaningfully" (p. 145). Second, and more fundamentally, he criticizes cognitivism for being mechanistic in its contiguity-based account of perceiving at physical and temporal distances. In this account, perception requires contiguity in time and space, hence the positing of psychical sensations in the mind (or physical sensations in the brain) that supposedly fill the distal and temporal gaps between objects and subsequent action (Costall, 1984; Marr, 1983). Such theories are unnecessary: We perceive directly.

Aside from problems in comprehensibility, Reed's chapters are flawed in their praise for Chomsky's (1959) review of Skinner's (1957) *Verbal Behavior* and for offering spurious and dated criticisms of behavior analysis as an associationistic, mechanistic S-R psychology in which, for instance, a word is "simply a stimulus"—presumably a physical stimulus that forces a physical response. And, as for perception, Reed asserts that in the behavior-analytic view "one does not perceive an object until one has made a discriminative response to it" (p. 145). Silly stuff, this.

These criticisms display Reed's lack of familiarity with contemporary radical behaviorism, especially its contextualistic rendering of behavior (and perception) not as mechanical responses (Schnaitter, 1987), but as "acts" (Lee, 1986, 1987) wherein stimuli and responses are functionally defined and interrelated (Skinner, 1935). Reed also overlooked automatic reinforcement

(Vaughan & Michael, 1982), stimulus equivalence (Sidman, 1986), conditioned "seeing" (Skinner, 1953, pp. 257–282), and what behavior analysts mean when they speak of perceiving (e.g., Goldiamond, 1962; Knapp, 1987), and thereby missed material that would obviate much of his criticism.

The other two chapters on Gibson by Katz and by Noble were clearer and more readable; indeed, the entire Gibson section might be read to better effect if these chapters were placed ahead of Reed's. Katz makes the case that although Gibson's psychology appears to be "realist," it is actually relativist (e.g., contextualistic). Realism entails, first, the ontological assumption that an external world exists separately and autonomously from the perceiver and, second, the epistemological assumption that what one perceives of the world is true of the world.

In contrast, relativism makes neither assumption, and neither does Gibson, and neither does behavior analysis. According to Gibson, subject and object stand in relationship to one another. They are mutually defining and interdependent; the environment cannot be specified independently of the organism. Katz does not deny that a physical world exists prior to and apart from the organism, but rather points out that the world the organism perceives and otherwise interacts with is the world of "affordances." Likewise, in behavior analysis, stimulus and response functions are defined interdependently; the function of one cannot be known without the other. In this sense, the function of a stimulus for a response is internal to their relationship, not external to it.

As for epistemology, perceiving is a matter of circumstances—the "circumstances" of organism and environment in context. More specifically, perceiving is a behavioral relation, and behavioral relations are a function of their historical context and current setting. Perceiving, then, as in "perceiving the truth," can never step outside itself—that is, outside the stream of behavior—for some absolute, veridical evaluation because that too (i.e., "perceiving the truth") is a behavioral relation in context, and so on. What an organism perceives, then, just as how it otherwise behaves, is always right.

In his chapter, Noble argues that a complete ecological psychology must encompass both perception *and* language. In so doing, he reiterates many of the points above: (a) Gibson's opposition to mainstream representational accounts of perception (cf. Skinner, 1974); (b) his philosophical pragmatism and phenomenology (cf. Day, 1969a, 1980); (c) his interest in functional, not structural, accounts of perceptual "systems," wherein neither account detracts from the purposes of the other (Catania, 1973); and (d) his approach towards language as social and functional, as opposed to logical and structural (cf. Skinner, 1957).

The radical tradition. Gibson's psychology, of course, had precursors and parallels in what Costall and Still call the "radical tradition of anti-dualism," which is offered in the second subsection of this part of the book. Among the precursors are the functionalism and pragmatism of James, Dewey, and Mead; among the parallels are phenomenology, Hegelian dialectics, and two versions of behaviorism, each covered in a chapter apiece. Not to have included behavior analysis here seems badly amiss, especially when it is contextualistically construed (e.g., Hayes, Hayes, & Reese, 1988; Morris, 1988).

Still's chapter on Tolman is perhaps the most original, scholarly, and accessible (read: readable) contribution to the book. His argument is that contemporary cognitive psychology is not heir to Tolman's purposive behaviorism, as is commonly presumed (see, e.g., Wasserman, 1981), but rather is heir to the Hullian S-R tradition. According to Still, Tolman sought to avoid both mentalism and reflex mechanism through a blend of the empiricism and phenomenology of James, Perry, and Holt.

This blend produced, for the young Tolman, the insight that purpose is "perceived directly" in behavior, and not inferred from it. That is, some aspects of some behavior in context set the occasion for calling it purposive. More technically,

"purpose" is a tact for certain acts-in-context. Thus, for Tolman, purpose was not an inferred cognitive state (i.e., hypothetical construct), but rather an intervening variable in the sense that it summarized a variety of stimulus-response relationships (cf. MacCorquodale & Meehl, 1948). Cast more broadly, Tolman's early positivism was not a logical positivism (Smith, 1986); it was more the descriptive positivism of Ernst Mach (Marr, 1985) and the epistemology of Ludwig Wittgenstein (Costall, 1980; Day, 1969b; Deitz & Arrington, 1984).

In Still's account, Tolman's early attempt to stand the middle ground between mentalism and reflex mechanics, and to transcend psychological dualism, failed for two reasons. First, out of convenience and conversational necessity, he adopted the mechanistic S-R "language-game," down whose slippery slope he eventually fell, into a mechanistic theory that required a mediating cognitive account to make sense of S-R relationships that were inconsistent at an observational level. Purpose and cognition were no longer intervening variables, but rather had become hypothetical constructs and mechanisms, often grounded in neurology (Tolman, 1949, p. 49; 1959, p. 114). This is cognitive psychology (see Morris, Higgins, & Bickel, 1982).

The second reason Tolman failed, if I read Still correctly, is that Tolman adopted the logical positivist account of science in which scientists are viewed as objectively detached from behavior as their subject matter. One consequence of this detachment is that behavior becomes an index of, or basis for making *inferences* about, some "true" behavioral reality. Scientists who adopt the logical positivist distinction between the knower and the known as their model of scientific inquiry often then presume that their subjects (knowers) operate in the same manner with respect to their own worlds (the knowns). That is, their subjects (e.g., rats) are also thought to make inferences about some true environmental reality, inferences that are for instance stored as cognitive maps or that are formalized as representations and rules.

This logical positivist account of science differs from the pragmatic, behavior-analytic perspective in which a scientist's effectiveness at describing, predicting, and experimentally controlling the subject matter is the truth criterion for an inductively derived theory (see Smith, 1986). In this view, scientific inferences are inferences about lawful behavioral relations at the level of behavior (Skinner, 1950); moreover, these inferences are themselves a behavioral relationship between the behavior of the scientist and the subject matter.

Tolman's story has been told in other ways in other places (Amundson, 1983; Smith, 1982). In those accounts, the later Tolman is taken as the precursor of cognitive psychology, and rightly so. In Still's account, however, Tolman never meant to be a mentalist or reflex mechanist, but simply lost the struggle to be otherwise. Hull (1930), in turn, was always and consistently both, and hence better fitted to be the father of cognitive psychology.

In his chapter, N. Smith points out that the behaviorism that is contrasted with cognitive psychology is little contrast at all—both are mechanistic. However, the behaviorism he offers—J. R. Kantor's interbehavioral psychology—does stand in sharp relief because it is field-theoretic and contextualistic in world view. Material on interbehavioral psychology is often enough published in this journal that it needs no further elaboration here (see, e.g., Moore, 1984; Morris, 1984; Parrott, 1984), but Smith does raise one issue that bears a comment: He distinguishes between "remembering" and "recall." Although cognition is the province of stimulus control in behavior analysis (e.g., Sidman, 1978), distinctions among different cognitive activities (e.g., between remembering and recall) are no distinctions at all if stimulus control generically accounts for them all. If what we tact in our natural language as different cognitive acts-in-context (Skinner, 1989) reflect different controlling relationships, these will eventually require some behavior-analytic attention (see Deitz & Arrington, 1984).

Platonic-Cartesian and the Hegelian

views are contrasted in a chapter by Markova, who argues that the former has historically treated concepts as static and "being," whereas the Hegelian framework is evolutional, developmental, and "becoming." Perhaps Markova's most insightful point is that evolution, development, and "becoming" do not arise from an asymmetric interaction between one thing changed (e.g., responses) with another thing unchanged (e.g., the environment), but rather through strong, reciprocal interactions where the functions of both are changed.

Finally, Bolton points out that what cognitive psychologists take phenomenology to mean is not what phenomenologists mean by it. Cognitive psychologists assign "priority to the subject," whereas phenomenologists assign priority to neither subject nor object: Phenomenology is no more a form of subjectivism than of realism, but of the interaction between subject and object. In accord with contextualism, Bolton's phenomenological approach to the truth has a touch of the pragmatic to it. That is, truth is not adduced within the organism as a matter of correspondence between representation and data, as in the mechanistic world view, but rather is derived from being-in-the-world: "There is no validation of shared concern other than shared concern" (p. 248). Read another way: There is no validation for the worth of basic or applied research other than effective action on the part of the individual or social group.

CONCLUSION

What are we to make of an edited book that questions cognitive psychology, none of whose authors is a behavior analyst? One answer: We may not be so few after all. Of a book that offers critiques of and alternatives to cognitive psychology, none of them behavior analytic? One answer: Other grounds beyond our own exist for criticizing and coping with a pernicious intellectual tradition. Of a book that contains chapters criticizing behavior analysis in a misinformed fashion? One answer: The authors' criticisms of mechanistic behaviorism were not wrong. What was wrong was their presumption that behavior analysis is mechanistic. In an important sense, this is not exactly the authors' fault: We sometimes forget that misunderstanding behavior analysis is behavior that is lawful. The behavior of these authors may have generalized to some formal similarities behavior analysis shares with mechanistic behaviorism (e.g., the stimulus-response language game). The authors simply do not know that the historical antecedents of the two traditions are different (see Day, 1980; Morris, 1988), and thereby fail to classify them separately. We will need to teach that discrimination.

I close on a different note, however, as did Costall (1984), with a quotation from Edwin Holt (1915), J. J. Gibson's mentor, whose advice to those seeking an alternative to cognitive psychology in 1915 remains apt today:

> It should be obvious that a fundamental unity of purpose animates the investigators of these several groups, although they approach the question of cognition from very different directions. Will it not be a source of strength for all if they can manage to keep a sympathetic eye on the methods and discoveries of one another? (p. 208)

REFERENCES

Amundson, R. (1983). E. C. Tolman and the intervening variable: A study in the epistemological history of psychology. *Philosophy of Science, 50,* 268–282.

Catania, A. C. (1973). The psychologies of structure, function, and development. *American Psychologist, 28,* 434–443.

Catania, A. C. (1978). The psychology of learning: Some lessons from the Darwinian revolution. *Annals of the New York Academy of Sciences, 309,* 18–28.

Chomsky, N. (1959). Review of Skinner's *Verbal Behavior. Language, 35,* 26–58.

Costall, A. (1980). The limits of language: Wittgenstein's later philosophy and Skinner's radical behaviorism. *Behaviorism, 8,* 123–131.

Costall, A. (1984). Are theories of perception necessary? A review of Gibson's *The ecological approach to visual perception. Journal of the Experimental Analysis of Behavior, 41,* 109–115.

Day, W. F. (1969a). Radical behaviorism in reconciliation with phenomenology. *Journal of the Experimental Analysis of Behavior, 12,* 315–328.

Day, W. F. (1969b). On certain similarities between the *Philosophical Investigations* of Ludwig Wittgenstein and the operationism of B. F. Skin-

ner. *Journal of the Experimental Analysis of Behavior, 12,* 489–506.
Day, W. F. (1980). The historical antecedents of contemporary behaviorism. In R. W. Rieber & K. Salzinger (Eds.), *Psychology: Theoretical-historical perspectives* (pp. 203–262). New York: Academic Press.
Deitz, S. M., & Arrington, R. L. (1984). Wittgenstein's language games and the call to cognition. *Behaviorism, 12,* 7–14.
Gibson, J. J. (1979). *The ecological approach to visual perception.* Boston: Houghton Mifflin.
Goldiamond, I. (1962). Perception. In A. J. Bachrach (Ed.), *Experimental foundations of clinical psychology* (pp. 280–340). New York: Basic Books.
Haugland, J. (Ed.). (1981). *Mind design.* Cambridge, MA: MIT Press.
Hayes, S. C., Hayes, L. J., & Reese, H. W. (1988). Finding the philosophical core: A review of Stephen C. Pepper's *World Hypotheses. Journal of the Experimental Analysis of Behavior, 50,* 97–111.
Hineline, P. N. (1984). Can a statement in cognitive terms be a behavior-analytic interpretation? *The Behavior Analyst, 7,* 97–100.
Holt, E. B. (1915). *The Freudian wish and its place in ethics.* New York: Holt.
Hull, C. L. (1930). Knowledge and purpose as habit mechanism. *Psychological Review, 37,* 241–256.
Knapp, T. (1987). Perception and action. In S. Mogdil & C. Mogdil (Eds.), *B. F. Skinner: Consensus and controversy* (pp. 283–294). New York: Falmer.
Lee, V. L. (1986). Act psychologies and psychological nouns. *The Psychological Record, 36,* 167–177.
Lee, V. L. (1987). The structure of behavior. *Behaviorism, 15,* 141–148.
MacCorquodale, K., & Meehl, P. E. (1948). Hypothetical constructs and intervening variables. *Psychological Review, 55,* 596–611.
Marr, M. J. (1983). Memory: Models and metaphors. *The Psychological Record, 33,* 12–19.
Marr, M. J. (1985). 'Tis the gift to be simple: A retrospective appreciation of Mach's *The science of mechanics. Journal of the Experimental Analysis of Behavior, 44,* 129–138.
Moore, J. (1984). Conceptual contributions of Kantor's interbehavioral psychology. *The Behavior Analyst, 7,* 183–187.
Morris, E. K. (1984). Interbehavioral psychology and radical behaviorism: Some similarities and differences. *The Behavior Analyst, 7,* 197–204.
Morris, E. K. (1988). Contextualism: The world view of behavior analysis. *Journal of Experimental Child Psychology, 46,* 289–323.
Morris, E. K., Higgins, S. T., & Bickel, W. K. (1982). Comments on cognitive science in the experimental analysis of behavior. *The Behavior Analyst, 5,* 109–125.
Parrott, L. J. (1984). J. R. Kantor's contribution to psychology and philosophy: A guide to further study. *The Behavior Analyst, 7,* 169–181.
Schnaitter, R. (1987). Behaviorism is not cognitive and cognitivism is not behavioral. *Behaviorism, 15,* 1–11.
Schwartz, B., Schuldenfrei, R., & Lacey, H. (1978). Operant psychology as factory psychology. *Behaviorism, 6,* 229–254.
Sidman, M. (1978). Remarks. *Behaviorism, 6,* 265–268.
Sidman, M. (1986). Functional analysis of emergent verbal classes. In T. Thompson & M. D. Zeiler (Eds.), *Analysis and integration of behavioral units* (pp. 213–245). Hillsdale, NJ: Lawrence Erlbaum.
Skinner, B. F. (1935). The generic nature of the concepts of stimulus and response. *Journal of General Psychology, 12,* 40–65.
Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review, 52,* 270–277, 291–294.
Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review, 57,* 193–216.
Skinner, B. F. (1953). *Science and human behavior.* New York: Macmillan.
Skinner, B. F. (1957). *Verbal behavior.* Englewood Cliffs, NJ: Prentice-Hall.
Skinner, B. F. (1969). *Contingencies of reinforcement: An operant analysis.* Englewood Cliffs, NJ: Prentice-Hall.
Skinner, B. F. (1974). *About behaviorism.* New York: Knopf.
Skinner, B. F. (1977). Why I am not a cognitive psychologist. *Behaviorism, 5,* 1–10.
Skinner, B. F. (1988). Comments. In A. C. Catania & S. Harnad (Eds.), *The selection of behavior. The operant behaviorism of B. F. Skinner: Comments and consequences* (p. 428). New York: Cambridge University Press.
Skinner, B. F. (1989). The origins of cognitive thought. *American Psychologist, 44,* 13–18.
Smith, L. D. (1982). Purpose and cognition: The limits of neorealist influence on Tolman's psychology. *Behaviorism, 10,* 35–48.
Smith, L. D. (1986). *Behaviorism and logical positivism: A reassessment of the alliance.* Stanford, CA: Stanford University Press.
Tolman, E. C. (1949). Discussion (from Interrelationships between perception and personality: A symposium). *Journal of Persönality, 18,* 48–50.
Tolman, E. C. (1959). Principles of purposive behavior. In S. Koch (Ed.), *Psychology: A study of a science* (Vol. 2). New York: McGraw-Hill.
Vaughan, M. (1987). Rule-governed behavior and higher mental processes. In S. Modgil & C. Modgil (Eds.), *B. F. Skinner: Consensus and controversy* (pp. 257–264). New York: Falmer.
Vaughan, M. E., & Michael, J. L. (1982). Automatic reinforcement: An important but ignored concept. *Behaviorism, 10,* 217–227.
Wasserman, E. A. (1981). Comparative psychology returns: A review of Hulse, Fowler, and Honigs's *Cognitive processes in animal behavior. Journal of the Experimental Analysis of Behavior, 35,* 243–257.